Response to Dr. Heymsfield's comment

We thank Dr. Heymsfield for his constructive comments (**in bold** hereafter). We have made changes and our responses are given below. The referee's comments are shown in bold with our responses in plain text.

This is a very nicely prepared article that presents observations in relatively shallow wave clouds where the temperatures are evidently too warm for ice nucleation to occur. The focus of the study is more on dynamics than microphysics. In that connection, the Introduction is beautifully written and very informative. Although the microphysics data are presented, it is not emphasized nor interpreted. I think this should be clearly stated at the end of the introduction section.

If the focus is on the dynamics, I suggest that the authors add additional calculations. I would like to see the calculation of latent heat release and warming by the growing droplets in the upwind part of the cloud and the cooling in the evaporative part. This could influence the dynamics, obviously. Also, it is important to look at whether there is drop survival between successive wave clouds to see whether this might explain the large drizzle-size drops that are observed with the 2DS.

If the focus is also on the microphysics, I suggest a few additions. Can the liquid water contents be explained from the observations, and which one of the three LWCs are reliable; the JW, Nevzorov, or CDP. The first two agree fairly well, the third one not. Can you account for the large drizzle drops. Are there CCN measurements; given the drop concentrations and updrafts, can we account for the drop concentrations?

The focus is not on the dynamics. Certainly it would be possible to estimate the latent heat release based on the microphysics data gathered at a penetration level. However, in order to produce a meaningful picture of the effects on the dynamics of heating or cooling, a modelling study is required which could provide vertical profiles. This is beyond the scope of this short paper. In the future, numerical simulations could provide better information on the latent heat release, the evaporative cooling, as well as the explanation of the drizzle-sized drops. We did not have instruments to measure CCN.

As for the comparison of the LWCs from different probes, we added the following in Section 2.1.

"The performance of Nevzorov and CDP probes have been examined previously (e.g., by Korolev et al., 1998, Isaac et al., 2006; Schwarzenboeck et al., 2009; Lance et al., 2010). Korolev et al. (1998) found that the integrated collection efficiency of the Nevzorov probe for the sensing of LWC varied between 0.9 and 1 without drops larger than 100 μm in diameter, but the efficiency for drops smaller than 5 μm could be as low as 0.6. Schwarzenboeck et al. (2009) further studied the response of the Nevzorov hot wire probe and found that droplets smaller than 20–30 μm partly tended to curve around the LWC sensor. Issac et al. (2006) pointed out that the Nevzorov Total Water probe and other similar hot-wire sensors provide underestimates possibly of liquid water content in the presence of large drops. Korolev et al. (1998) estimated an absolute accuracy of the Nevzorov LWC probe to be 10%–15% for liquid droplets in the size interval 10–50 μm. Lance et al. used glass beads and polystyrene latex spheres for calibration. They found that the CDP oversized droplets smaller than 20 µm and a better agreement with a 2 µm offset."

We added the following at the end of Section 3.
"The values of LWC are generally higher from the CDP than from the Nevzorov and the Johnson-Williams probes. The discrepancy is very much likely due to the reduced collection efficiency of the Nevzorov and the Johnson-Williams probes (for the reasons highlighted in section 2.1) and by the slight over-sizing of droplets (by up to 2 µm) by the CDP when the equivalent diameters of the drops were predominantly smaller than 45 µm in the clouds."

Specific comments appear below.
Pg. 13339, line 9. Three reasons, the third being instrument evaluation which would have been nice to have done in your study.

We added "Finally, the laboratory-like nature of wave clouds make them useful for instrument evaluation."

line 14. Heymsfield and Miloshevich (1995) studied the ice production in wave clouds. And, a number of recent papers in JAS about the ICE-L experiment-Heymsfield et al. and Field et al.

These papers have been added in the text and reference sections.

Pg. 13340, line 8. I don't understand the 90% rather than 100%.

The unperturbed humidity is ~90% prior to being perturbed by the waves. The upward (downward) motion induced by the waves will increase (decrease) the real humidity such that in the wave crests the relative humidity reaches supersaturation allowing clouds to form. We added the above to the text to clarify the point.

Line 20. These were wave clouds, not cirrus.

This has been changed.

Line 23. The maximum RH in that case was governed by homogeneous ice nucleation.

We changed the sentence to "The relative humidity, which was governed by homogeneous ice nucleation, attained its maximum in the middle of the cloud where the temperature was lowest."

Pg. 13341, last sentence first paragraph. Weren't such measurements made in the T-REX project over the Sierra in 2006?

We changed "...it is possible to measure the horizontal structure of mountain wave clouds in terms of thermodynamics and microphysics." to " ...it is possible to measure the horizontal structure of mountain wave clouds in terms of thermodynamics and microphysics, such as in the T-Tex project (Grubišić et al., 2008)."

Grubišić, V., Doyle, J. D., Kuettner, J., Dirks, R., Cohn, S. A., Pan, L. L., Mobbs, S., Smith, R. B., Whiteman, C. D., Czyzyk, S., Vosper, S., Weissmann, M., Haimov, S., De Wekker, S. F. J., and Chow, F. K.: The Terrain-Induced Rotor Experiment A Field Campaign Overview Including Observational Highlights. Bull. Amer. Meteor. Soc., 89, 1513-1533, 2008.

Section 2.1. Suggest including only those instruments you used in this study.

The CPI data do not directly appear in the figures or tables in the manuscript, but the data were used in conjunction with data from other instruments to check the boundaries between cloudy and clear air, the presence of drizzle drops, and the non-existence of ice particles.

Pg. 13342, line 11. “Well-defined”. This is subjective and unfounded. Perhaps well studied or something like that.

"well-defined" has been deleted.

Line 15. I absolutely do not buy into the idea that phase can be readily/accurately determined from the 2D-S. In fact, you need to include some examples to demonstrate this.

We have deleted "of both phase".

lines 15 and thereafter. Were there anti-shattering (Korolev) type tips on any of these probes?

There were no anti-shattering (Korolev) type tips on any of these probes for this flight. We added the above sentence to the text.

Pg. 13343, Fig. 4. Why is there such a mismatch between the LWC measurements? Can you estimate the “true” LWC from your upwind RH data and examine the discrepancy?

Please see the reply to the general comment where we added a discussion of the mismatch between the LWC measurement. It is not possible to estimate the true LWC in a reliable way since we did not have the trajectories of the air parcel and we did not know the magnitude of the moisture which is affected by the entrainment/mixing process.

Pg. 13344, line 4. Also, see: Heymsfield, A. J., J. E. Dye, and C. J. Biter, 1979: Overestimates of entrainment from wetting of aircraft temperature sensors in cloud. J. Appl Meteor., 18, 92–95.

The paper has been added to the text and references section.

Last paragraph. The overlap between the CDP and 2DS PSDs compare favorably. You may want to point this out.

We added ", which demonstrates favourable agreement in the overlap region of the size distribution." after "... for run 1 are shown in Fig. 6".

Pg. 13345, line 7. Well, this is clearly what is expected. It would be a problem is you did not find this result.

We agree.

Pg. 13347, lines 2-3. The sentence needs restructuring.

It has been changed to "We found in our study that the maxima in updraughts are on the upshear side and the strong downdraughts are on the downshear side."

Lines 10-12. Is there droplet survival between clouds? This should be checked from the 2DS data and it could be important for explaining the large, 2DS-size drops.

We have checked the 2DS and CPI data. There were no drops observed in the gaps between the cloud regions .

Pg. 13348, line 2. Are you able to estimate the contribution of latent heating by cloud droplets to the upshear side and the relative contribution of evaporation to the downdrafts on the downwind side?

This is an interesting point. It would be useful to study this with a model with detailed microphysical processes on a high resolution grid. This was not possible for the current paper, but a future modelling study is planned.

Line 5. Wave clouds also have been used to test and evaluate instrument performance. It would be interesting to briefly mention whether the LWCs measured by the different probes are consistent with the LWC estimated for cloud base conditions or on the leading edge of the cloud.

We did not estimate the values of LWC in the clouds because it is difficult to get accurate values. As discussed by Smith (1990), the water content of an air parcel is affected by advection and convection and air parcel, turbulent diffusion, cloud formation and dissipation, and precipitation. The uncertainty of estimating LWC from humidity is not clear. Instrument evaluation was not the focus of this paper.

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